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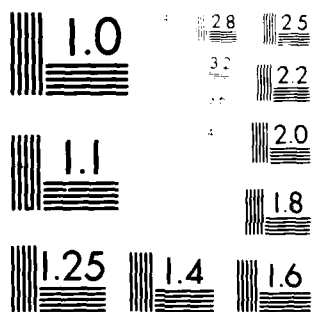
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SOME PHILOSOPHIES OF INFERENCE
AND MODELLING

Tom Leonard

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MATHEMATICS RESEARCH CENTER

SOME PHILOSOPHIES OF INFERENCE AND MODELLING

Tom Leonard

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ABSTRACT

During the Spring semester of 1981, the Mathematics Research Center held a weekly statistical discussion series as a precursor to its special year on Scientific Inference, Data Analysis, and Robustness. The many discussants included G. E. P. Box, D. V. Lindley, B. W. Silverman, A. Herzberg, C. F. Wu, B. Joiner and D. Rubin. Many aspects of statistics were discussed, including the Box philosophy of deductive and inductive reasoning, and Lindley's coherent Bayesian viewpoint. The present paper attempts to constructively review the discussion series, and to add a number of retrospective comments and suggestions.

AMS (MOS) Subject Classifications: 62-06, 62A15

Key Words: Modelling, Inductive, Deductive, Bayes, Frequentist,
Significance Testing

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SIGNIFICANCE AND EXPLANATION

There are two main types of statistical reasoning. Deductive reasoning is concerned with inferences conditional upon the truth of the model, whilst induction relates to model formulation and scientific discovery. During the MRC Statistical Discussion Series, in the Spring of 1981, a variety of aspects of this and related philosophies were discussed. Topics covered include Checking Models, the Likelihood Principle, Principles on Model Space, Significance Testing, Scientific Discovery, Data Analysis, Randomization, Robust and D-Optimal Designs, Data-Handling, Subjective Probability for No-Data Problems, How Statistics Should Be Taught e.g. on Short Courses, Sequential Analysis, Assessing Prior Predictive Distributions, Rounding Errors in Regression, Exchangeability in Statistics, the Future of Statistics, and Statistical Ethics. In the present paper these discussions are critically reviewed, and some further suggestions made.

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SOME PHILOSOPHIES OF INFERENCE AND MODELLING

Tom Leonard

SECTION 1: SESSIONS 1 TO 3 WITH FURTHER IDEAS ON MODELLING, SIGNIFICANCE TESTING AND SCIENTIFIC DISCOVERY.

Session 1: Checking Models, George Box (1/23/81)

In the first session the deductive and inductive aspects of statistical investigation were discussed. Deduction is appropriate for inferences upon the truth of the model, whilst inductive thought is necessary during model checking. During the semester it became apparent that all serious discussants were in agreement on this issue

There was a bit less agreement on which philosophy should be employed during the model checking procedure. Discussants seemed to split into the following three main areas:

(a) Bayes is good for inferences given the model but frequentist procedures, e.g. significance tests, are necessary when checking the model.

(b) Bayes is good for inferences given the model, and Bayes is also good for model-checking (e.g. prior distributions on either sampling densities or different models or polynomial coefficients) but more Bayesian theory needs to be developed in the model-checking area.

(c) Frequentist procedures are adequate for both inferences and model-checking.

The main debates were between (a) and (b). Frequentist model-checking needs few assumptions about alternative

models, whilst Bayesian assumptions always reduce to a grand model involving models across models. Therefore frequentist model-checkers can point to the simplicity and generality of their approach, whilst Bayesians could give the response that it is always necessary to inject a certain amount of structure into the analysis in order to focus upon precise conclusions. This is an important issue which was largely unresolved.

The Inductive Modelling Process (IMP) and the subsidiary importance of coherence were discussed, with responses by D. V. Lindley, A. F. M. Smith, and others, in my paper in the recent volume on "Bayesian Statistics" issued by the University of Valencia Press.

Session 2: The Likelihood Principle, Tom Leonard (1/30/81)

In the second session the Likelihood Principle was introduced in the context of making inferences conditional upon the truth of the model, and the proof of Birnbaum's theorem was presented. This proves that if the statistician accepts the sufficiency and conditionality principles (which are open to straightforward frequentist interpretations) then he must accept the Likelihood Principle, conditional upon the truth of the model, and should not therefore employ any approach involving integrations across the sample space (e.g. UMVU estimation, confidence intervals, significance tests).

The reaction to these ideas was interesting. Bayesians viewed the sufficiency and conditionality principles as obviously acceptable. Traditional significance testers felt that, since the Likelihood principle and testing are not compatible, there must be something misleading in the underlying assumptions (most likely the Conditionality principle). Another expressed view was that the Likelihood Principle is largely irrelevant since it conditions on the truth of the model, whilst most of the statistician's effort needs to be spent on model-building. One nice interpretation was that "if your analysis does not satisfy

the Likelihood Principle than this means that your model is wrong".

Overall, it seemed that few existing views were changed by this exposure to Birnbaum's theorem. This may be viewed as surprising, as the proof and underlying assumptions for this theorem are extremely plausible and simple.

Session 3: Tying together the ideas of the last two sessions, Dennis Lindley,

In the third session an attempt was made to extend the Likelihood Principle ideas from the inferential to the modelling situation. This involved a prior distribution across model space, and the ideas therefore needed to be partly interpreted from a Bayesian point of view. The main reactions were either (a) a Likelihood Principle would be neither reasonable nor desirable in modelling situations since frequentist ideas are obviously more appropriate on model space, or (b) these ideas would be desirable in modelling situations but some further theoretical development would be needed in order to obtain a modelling principle with the same impact for non-Bayesians as the Likelihood Principle for inference.

Perhaps the discussants in Session 3 might have favourably considered the following principle:

The Modelling Principle (special case of Sufficiency Principle and of the Likelihood Principle)

Suppose that the outcome of an experiment is the numerical realization x taking values in a sample space H . Let $f_1(\cdot)$ and $f_2(\cdot)$ be two probability densities defined on H (with respect to an appropriate dominating measure) such that

$$f_1(x) = f_2(x)$$

Then unless there is information external to the data to suggest otherwise, neither of f_1 and f_2 should be viewed as preferable for modelling conclusions based on x .

N.B. For each i , $f_i(x)$ is a probability density, conditional on the unknown "parameter" θ_i . This may also be interpreted as the likelihood functional of f_i

conditional on \underline{x} . The Modelling Principle is saying that if f_1 and f_2 possess the same likelihood functional then they should be viewed as equally preferable for modelling conclusions based on \underline{x} , in the absence of external information.

The Modelling Principle could be used to critically interpret well-known modelling approaches due to Tukey and Parzen. Whilst Box's modelling approach is particularly well-formulated; the following example is interesting. Note that problems with events of probability zero, whilst unimportant, could be removed by extending the Modelling Principle to say that, in the absence of external information, f_1 should be preferred to f_2 whenever $f_1(\underline{x}) > f_2(\underline{x})$.

Example - Box's Modelling Approach

Consider an observation vector $\underline{x} = (x_1, \dots, x_n)^T$ assuming values in the sample space Ω which we take to be n -dimensional Euclidean space R^n . Suppose that

$q(\cdot) : R \rightarrow R$ is some monotonic transformation on the real line (e.g. a Box-Cox transformation). Assume further that the observed elements of \underline{x} happen to satisfy the specific condition

$$\sum x_i^2 = \sum q^2(x_i) - 2 \sum \log \left| \frac{\partial q(x_i)}{\partial x_i} \right| = S^2 \quad (*)$$

Consider the alternative models M_1 and M_2 specified by M_1 : The x_i are realizations of independent random variables x_i which possess standard normal distributions. The corresponding probability density is

$$f_1(\underline{x}) = \frac{1}{(2\pi)^{n/2}} \exp\left[-\frac{1}{2} \sum_i x_i^2\right] \quad \text{for } \underline{x} \in \Omega$$

M_2 : The x_i are realizations of independent random variables x_i such that the transformed variables $Y_i = q(x_i)$ possess standard normal distributions. The probability density is now given by

$$f_2(\underline{x}) = \frac{1}{(2\pi)^{n/2}} \exp\left\{-\frac{1}{2} \sum_i q^2(x_i) + \sum \log \left| \frac{\partial q(x_i)}{\partial x_i} \right| \right\}$$

for $\underline{x} \in \Omega$.

Note that under condition (*) $f_1(\underline{x}) = f_2(\underline{x})$ so that whenever \underline{x} satisfies (*), the Modelling Principle tells us to prefer f_1 and f_2 equally in the absence of external information.

The Box modelling approach tells us to discredit M_1 if the tail probability

$$\begin{aligned} \theta_1 &= p(f_1(\underline{x}) < f_1(\underline{x})) \\ &= p(\sum x_i^2 > s^2) \end{aligned}$$

is too small, where the probability on the right hand side arises from the distribution of \underline{x} under M_1 , and s^2 is specified in (*). Note that θ_1 is just the probability that a chi-squared random variable, with n degrees of freedom is greater than or equal to s^2 .

We should also discredit M_2 if

$$\begin{aligned} \theta_2 &= p(f_2(\underline{x}) < f_2(\underline{x})) \\ &= p(\sum x_i^2 - 2 \sum \log \left| \frac{\partial q(x_i)}{\partial x_i} \right| > s^2) \end{aligned}$$

is too small, where the probability on the right hand side is now based upon the distribution of \underline{x} under M_2 . Since $\sum y_i^2$ possesses a chi-squared distribution with n degrees of freedom, and this is adjusted by an extra function of \underline{x} , we see that θ_1 will not in general be the same as θ_2 . Therefore, although the Modelling Principle tells us to equally prefer M_1 and M_2 , we seem to arrive at different tail probabilities in each case.

Our overall conclusion is that Box's Modelling Approach and the Modelling Principle are not in mathematical agreement. This may be the source of some discussion.

Perhaps the philosophy "all principles are there to be broken, but in this case we may learn a great deal by considering why we have broken them" may be useful here.

Significance Testing

During the first three sessions a large amount of time was spent discussing the merits of significance testing. There was some measure of agreement that these are unreasonable given the model but much less agreement in the modelling situation. The main points raised by objectors to significance tests were

(a) Fixed size tests are fairly arbitrary and it seems to be extremely difficult to interpret the magnitude of the p-value when so many different aspects like sample size, model complexity, and selective reporting affect the p-value.

(b) There is no justification for making accept/reject decisions based on significance tests.

(c) It is dangerous to summarize the results of an experiment by a single p-value.

(d) In modelling situations it is necessary to have alternatives in mind; standard tests for fit do not involve alternative models and may therefore not be based upon enough assumptions to facilitate useful conclusions.

Proponents of significance tests made the following points:

(a) The p-value can be interpreted very naturally either by thinking in terms of the tail area of the sampling distribution or by comparison with the p-values of other experiments. Interpretations based upon surprise factors are particularly important.

(b) When the majority of effort is spent on model-building it then seems rather unimportant to argue about the difference between 5% and 4% at the end of the analysis.

(c) The p-value is only one of a large number of aspects which a statistician should think about in reaching his conclusion. It is not a formal mechanism e.g. for

decision making, but simply a valuable guide to the inductive thought processes.

(d) When checking a model it is impossible to have all possible alternatives in mind, and therefore any procedure which conditions upon alternative models must be inadequate, thus, for example, ruling out any Bayesian procedure.

In summary, whilst tests for fit might be viewed as more appropriate than tests for parameters within a model, the big question is whether or not they indeed produce the goods i.e. do they provide a completely acceptable procedure for model-checking in the absence of alternative hypotheses, or is more structure needed in order to arrive at really convincing conclusions? In other words, can p-values for tests for fit be interpreted in a meaningful way, or is it simply too ambitious to hope to check a model unconditionally upon possible alternatives? My personal opinion would be a bit on the negative side but I would be prepared to be convinced either way. I challenge significance testers present to advise me how they actually make a practical judgement about a p-value; I remain unconvinced that they do much more than think in terms of 1% and 5%.

DISCOVERY AND INSIGHT AS OBJECTIVES OF THE SCIENTIFIC METHOD

A primary purpose of statistics is to discover new real-life conclusions e.g. a possible association between important medical factors, new chemical components useful in, say, agriculture, or novel ways of stimulating the economy. Statistics also plays a partly confirmatory role, but this is secondary to discovery. I view insight as closely related to discovery, and insight and discovery are perhaps of equal importance. Inductive modelling combined with local deduction takes statistics out of the unreasonable restrictiveness of the Neyman-Pearson and coherent Bayesian areas, and into the forefront of science, as an important vehicle for insight and discovery.

Professor Box prefers a Bayes/frequentist compromise as a means of describing his deductive and inductive reasoning.

I prefer a pragmatic Bayes/pragmatic Bayes compromise. I would for example always try to work with at least the conceptual background of a prior distribution across the space of sampling models, and perhaps to employ a pragmatic short-cut to approximate to a full blown non-parametric Bayesian procedure. For example, Schwarz's criterion provides an excellent pragmatic method for judging the degree of a polynomial approximation to a non-parametrised regression function or sampling density. In short, I have developed my own pragmatic Bayes/non-parametric Bayes procedures for coping with modelling situations, and these will be reported in detail elsewhere. It is for example possible for the statistician to introduce a hypothesized model as prior estimate, and then to let the data help him to find possible deviations from his hypothesized model. This ties in well with the deductive/inductive scheme. (See my course notes on Bayesian Inference and Modelling.)

Unlike Professor Box, I do not view the prior distribution of the parameters as part of the sampling model. Under a Bayesian non-parametric procedure there is no restriction to the type of sampling model which can be considered or to the type of discovery which can be made. I however find Professor Box's frequentist compromise to be of potential importance both in stimulating tremendous input into the modelling area, and in suggesting that we should check the reasonability of the prior (e.g. in its tails) as well as the reasonability of the sampling model. These are of course two separate problems. Perhaps the prior should be checked by investigating the properties of the estimates it leads to.

SECTION 2: REVIEW OF SESSIONS 4-13

Session 4, Some Thoughts on Data Analysis,

Bernard Silverman (4/13/81)

In the fourth session the presentation of statistical data was discussed, and a method based upon kernel estimators was proposed for representing a random sample by a smooth curve. Whilst this provides concise

representations, some of the information in the sample will be lost. There was a debate about the merits of kernels and histograms, with histograms gaining a slight advantage.

During this session there was also a debate about whether anyone had ever actually analysed a random sample. The consensus of opinion seemed to be that whilst some random samples have at times occurred in designed experiments, most samples have non-random characteristics.

Session 5, Randomisation, Jeff Wu (2/20/81)

In the fifth session the merits of randomisation were debated. It seemed to be the general opinion of both Bayesians and frequentists that randomisation is an invaluable device. It for example removes bias due to factors which would be difficult to model precisely, and also helps the statistician to cope with the problem of the lurking variable.

The only point of debate was whether the analysis should be carried out conditionally or unconditionally upon the actual design employed. This issue parallels the debate on the Likelihood and Conditionality Principles.

The problems of how to hunt out lurking variables, or how to analyse data in the presence of lurking variables is one of the most important real issues which statisticians are faced with, particularly when analysing, say, medical or economic data, rather than data from designed experiments. It should probably receive much more attention than, say, the frequentist/Bayes philosophy.

Session 6, Robust Designs, Agnes Herzberg (2/27/81)

In the sixth session robust designs were discussed with emphasis on the criterion of D-optimality. It was generally agreed that

(a) The theory of experimental design should always be mixed with practical common sense, and that a pragmatic design is often more useful than a theoretically optimal design, particularly when model inadequacies are taken into account.

(b) That the criterion of D-optimality is just one way of summarizing the elements of the $X^T X$ matrix based upon the X matrix for the assumed true model, so that D-optimal designs should be treated with a great deal of caution. Recent work by Toby Mitchell and C. F. Wu on robustification of designs may also be useful here.

Session 7, The Frontiers of Statistical Analysis,

Brian Joiner (3/6/81)

In the seventh session the main point discussed was whether it is useful to discuss slight differences between statistical methodologies when the most serious problem with large data sets is whether they have been collected properly, or stored properly on the computer, or whether it is possible to obtain convenient summaries of the data set for a preliminary analysis. A number of data sets were presented in order to illustrate various pitfalls that may be caused by careless data-handling.

There seem to be two separate problems here; clearly data handling merits considerable attention particularly when 90% of any statistical analysis should involve careful consideration of the data, for example using scatter plots and cross-tabulations. However, having done this we still need a decent formal analysis in order to sort out the statistical variation in the data. So good data handling and good statistical methodology are both of essential importance.

There seems to be some doubt as to the wisdom of collecting large quantities of badly handled data, when only a small proportion of it may ever get analysed. Perhaps the philosophy "The greater the amount of information the less you know", is not completely out of place here.

Session 8. Subjective Probability for Data Problems,

Jim Dickey (3/13/81)

In the eighth session we switched to subjective probability for no data problems; and discussed the elicitation of prior distributions from non-statistical experts. This is a growing area amongst a certain breed of

Bayesians, and there has been some progress for single parameter problems. However, severe difficulties are faced in multi-parameter situations because of the problem of quantifying the possibly nonlinear interdependencies between different parameters. So the problem shows some capability of solution, but needs considerable more development. Procedures suggested for ensuring coherence don't always seem to be completely coherent themselves e.g. there is often a heavy dependence on least squares.

Session 9. Education in Statistics, Conrad Fung (3/27/81)

A number of points relating to education in Statistics were discussed in the ninth session. It was for example felt that statistical teaching should relate both to current applications of our methods and to the future careers of our students e.g. in industry.

This seems to be of considerable importance because the statistics we are teaching now is the statistics which is going to be applied in industry, maybe for the next forty years. Perhaps we need a moratorium on all "bad" statistical methods (confidence intervals, and UMP tests?), so that only "good" methods (pragmatic Bayes?) survive into the next century.

Session 10, Sequential Analysis, Connie Shapiro (4/3/81)

In the tenth session the theory and practical relevance of sequential methods were discussed. The applicability of the Likelihood Principle was debated in the context of the variety of stopping rules available. Another important point is that, whilst an optimal Bayes solution is always available, the extensive analysis may be extremely computationally complicated so that only approximate rules are feasible. Also, in practical situations it is generally infeasible to make the assumptions necessary for sequential analysis, and a pragmatic rule will often work better. Furthermore, the advantages in using a sequential rule may be diminished when model inadequacy is taken into account.

Session 11, The Truth About Bayesian Inference,

Steve Stigler (4/10/81)

In the eleventh session, the feasibility of judging prior opinions via the predictive distribution was discussed, with historical references to Rev. Thomas Bayes' original paper. It was suggested that a serious difficulty is caused for Bayes because, for a given sampling distribution, there may be no prior distribution corresponding to the predictive distribution selected. However this simply means that the predictive and sampling distributions have not been chosen sensibly, and therefore provides a coherency check.

Session 12, Rounding Errors in Regression,

Don Rubin (4/13/81)

In the twelfth session we discussed an asymptotic Bayes method for rounding errors which makes opposite adjustments to those suggested by numerical analysis. This is because the posterior distribution of the rounding errors is not locally uniform since it incorporates knowledge of the regression line. This is an excellent example of a situation where Bayes and pragmatism can be mixed to good effect.

Session 13, Exchangeability in Statistics,

Dennis Lindley (4/24/81)

In the thirteenth session we discussed the idea of conditional exchangeability of observations as a Bayesian method for interpreting data. This for example leads to a resolution of Simpson's paradox. It also highlights the Bayesian theme that it is necessary to utilize information concerning the background of the data (e.g. when deciding which factor to condition on) if we are to have any hope of drawing meaningful conclusions from a finite number of observations

SECTION 3: REVIEW OF CLOSING SESSION, TOGETHER WITH
FURTHER IDEAS ON STATISTICAL ETHICS

Session 14, A Review Session of the Bull Sessions,
Tom Leonard (5/3/81)

My overall feeling is that an ideal statistician (a) relies on his common sense and pragmatic judgement, (b) gets involved in the scientific background of the data, (c) is prepared to use theory when it is likely to help him reach a useful conclusion, (d) is unwilling to accept any theoretical procedure unless he is convinced that it is practically relevant, (e) is at least partly Bayesian.

I would like to predict that in the next century statisticians will be one-third Bayesian, one-third data analyst, and one-third scientist, i.e. they will view statistical theory and practice and scientific background as a single entity.

I would like to conclude with some comments on the role of ethics in Statistics.

Statistical Ethics

A statistical procedure could be said to be ethical if it has a beneficial effect on the people (e.g. bourgeoisie) on whom it is likely to have an effect.

Ideas of ethnicity seem to be of growing importance in Statistics, for example in medicine and education. I think that the profession should view itself as responsible for developing ethical standards to cover the effects of statistics on ordinary people.

A procedure could be said to be irrelevant if it is not constructed with ethnicity in mind.

A procedure could be said to be unethical if it is constructed contrary to the definition of ethnicity.

Proposition: In the final analysis, the worthiness of any statistical procedure may be based solely upon consideration as to whether or not it is ethical.

Definition: A procedure possesses double standards if it purports to be ethical, but in fact and deed either irrelevant or unethical.

The primary example of procedures which possess double standards are the types of significance tests currently employed in, say, sociology or psychology, where whole professions can be misguided by the whims of the "objective" accept/reject philosophy at the 5% level. The following proposition might also be worth considering:

Proposition: Some of the very extreme forms of coherent Bayesian philosophy run the risk of possessing double standards unless they seek quick resuscitation from practical data, the scientific environment, and real statistics. At first sight, they provide us with all consuming theories. However, upon careful scrutiny, they are irrelevant and misleading in actual terms, and therefore have an unhelpful effect upon scientific investigation. A prime example is Bayesian Decision Theory which suffers from both the ambiguities of the Expected Utility Hypothesis and severe difficulties in basing the choice of loss function upon practical reasoning.

Finally, I would like to suggest a Bible for confirmed adherents of this sort of philosophy. This is "The Search" by C. P. Snow, and concerns the realities and unrealities of scientific investigation together with the unfortunate experiences in academia of a graduate student with bourgeois scientific attitudes and moral standards. It is my personal belief that people who identify with this student may well have just the right attitude towards scientific investigation. Perhaps we are all C. P. Snow's at heart.

Postscript: The statistical discussion series recommenced in the Fall of 1981, and new ideas were presented on the topics discussed above. The next six talks were:

"Some Approaches to Modelling" by Tom Leonard

"Time Series and Outliers" by George Tiao

"The Boundaries of Statistics" by Rob Miller

"Box's Modelling Approach for Bayes-Stein Problems" by Kevin Little.

"The Analysis of Finite Populations" by Jeff Wu
and

"The Analysis of Transformations Revisited; A Rebuttal"
by George Box with further talks planned by Don Rubin,
Ching-Shui Chenq, Dennis Cox, and Rick Nordheim

Tapes are available for all these talks, which include
many stimulating discussions together with a number of
humorous interludes.

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ABSTRACT (Cont.)

coherent Bayesian viewpoint. The present paper attempts to constructively review the discussion series, and to add a number of retrospective comments and suggestions.

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